

characteristics in behavior, frequency, etc. of a weed species. This condition is shared by *E. caroliniana* and, in part, by *E. pilosa* of this species group. Mr. Long has shown conditions and the distribution in the Philadelphia and adjacent region to be largely identical and has also shown a general relationship with the local region through several outlying stations, one of them from Lehigh county. It is strongly presumed from the observations and collections already made in the counties adjacent to Lehigh county that conditions in these areas will be found on more intimate investigation to be quite similar to those in Lehigh county. The general region northward toward and in the mountains beyond the limits of Lehigh county has not been touched and the outposts locally have not been reached. Thus there has been neither time nor opportunity to extend further the limits of occurrence of *E. peregrina* in the general local region. However in this brief note, apart from the specific conditions noted for Lehigh county, a general relationship with the Philadelphia region has been definitely indicated and it is hoped some information has been offered that will not only be useful in an understanding of the general relationship of this highly complex and difficult species group but that will be helpful as well in establishing the general distribution of *E. peregrina* in America.

ALLENTOWN, PENNSYLVANIA.

USAGE.

KENNETH K. MACKENZIE.

IN an article which has just appeared in RHODORA (28: 138) Mr. Weatherby touches on many points. He most earnestly and fully believes in his point of view and is therefore entitled to the fullest respect for his views. He deals with a number of different subjects. He fails to consider where his suggestions would lead, if applied. He is often delightfully vague. Some of the matters touched on are not of any general importance, but there are others which do very much deserve notice.

In the first place, it should be emphasized that the greatest curse which science has to deal with is laziness and mental inertia—the desire that because one has learned a thing in a certain way, that it should always remain that way. A matter so learned to an in-

dividual with this view-point has become "usage." He sets himself up from his own horizon to judge the world and illustrates the old view that every man makes himself a measure of the universe. Because a few books have become known to such an individual and have become the basis of his knowledge, he regards them as sacrosanct. Because for a few years and among a few scientists certain methods have been followed, he regards these as required in perpetuity by what he calls "usage." The law long years ago had similar views presented, and emphatically settled that much more is required to constitute good usage.

We each and all have deeply imbedded in us a desire not to have things to which we are accustomed changed. A well known scientist puts the matter very correctly in a letter in which he says "I do not suppose I have any right to complain personally, for I did a bit of it myself not so many years ago, but it is much more difficult to accommodate one's self to the changes made by somebody else." But if we are to have progress, we must view all suggestions of change with open mind and apply only one test to them; that is, whether they are correct or not.

I earnestly submit that real science should always, first, last and all the time aim to get at the truth. No question of convenience or anything else should stand in the way of getting at the truth. That is the one thing which should always and under all circumstances be aimed at. Errors of every kind should be searched out and corrected, regardless of whether finding and correcting them will make some individuals unhappy. Scientists all over the world have properly held up to scathing criticism and rebuke actions of the legislatures of southern states in prohibiting the teaching of evolution, but scientific circles averse to correction of their own errors—fighting for and desiring to hold to them on the ground of usage or any other euphemistic phrase for mental inertia—are just as blameworthy. And it may be noted here what these southern states are upholding to them is usage and usage which to them is of a sacred character. They have a basis of religious conviction for their action, which is entirely lacking among those scientists to whom usage is so important.

Another thing it is also earnestly submitted which is equally necessary and equally important, is the need to treat all scientists fairly and on the same basis. When scientists adopted the plan of quoting the authors of scientific names in connection with their names, they

adopted a plan, one of the purposes of which was to give some measure of enduring recognition to authors who rarely, if ever, got anything else out of a lot of hard work. But when such a plan is adopted, the fair and honest procedure is to see that each author is given credit for the work he does. When A proposes a genus or a species and later on B proposes the same genus or species under another name, the proper thing to do is to take up and use the first name. But here again we come up against the same mental inertia so often characteristic of scientific work. Some few botanists will have become acquainted with the work of B and not with the work of A or the work of B may have been put out by some large institution and the work of A not. Then the cry is at once set up that the work of B has become known through "usage" and his names must be placed in a list of *nomina conservanda* and always used, while the first work of A must be relegated to obscurity. One would think that it would be the easiest thing in the world to learn these earlier names once for all, and that the amount of labor in so doing would be infinitely less than the amount of labor and trouble involved in having some scientists using the names of B and some using the names of A. And to save a little trouble is the only reason for a list of *nomina conservanda*. But learning unaccustomed names is for some reason one of the hardest things which a certain type of scientist can bring himself to do—his entire nature calls on him to protect and cherish the names with which he himself has become familiar. Like the scenes of his childhood, they are part of his life, and woe be it unto anyone who in any way dares to attack them. But let anyone suggest that a name proposed by any such scientist himself should be arbitrarily legislated against and then see with what enthusiasm the suggestion will be received by such scientist!

Of the specific suggestions made by Mr. Weatherby the first to be noted is his very great over-emphasis on the value of specimens in old herbaria. Unfortunately this fails to take into account the situation with respect to these old herbaria and the extent to which the specimens are authentic. The questions involved have been carefully gone into by various scientists and their papers are readily accessible. Some years ago Dr. T. Holm (*Am. Journ. Sci.* (4) **15**: 145–152. 1903) went into the facts involved fully. The following quotations are from his article.

"The futile endeavor on the part of certain modern systematists to

verify plant-species, established by the earlier authors, by means of their herbarium-specimens but regardless of the diagnoses, has resulted in some very strange discoveries, so strange indeed that they are hardly to be believed. And the excuse for not considering the diagnoses is simply the belief that the herbarium-specimens are to be looked upon as "types" of the respective species. . . . It would appear at once that the verification of such old species means a good deal more than a hasty examination of the specimens, that no small amount of literary research is involved, a study of the author's method of describing, of citing, the history of the herbarium as it has been left at his death, etc. . . . There is no indication whatever to prove that the specimens preserved in these old herbaria are those that served as base for the diagnosis. . . . It is a well-known fact that a large number of the specimens collected by Linnaeus do not correspond with the diagnosis, written by himself. . . . It is, thus, evident that Linnaeus' species must be studied by means of his diagnoses and not from the specimens or quotations, and this is, of course, in many instances, quite a difficult task."

To follow Mr. Weatherby's intimations and identify Linnaean species by his specimens regardless of his descriptions, would result in an extraordinary number of changes of names. Dyckes (Genus *Iris*, p. 6) says about half the *Iris* specimens are incorrectly named. Gray (Proc. Am. Acad. 17: 177-9) found the *Solidago* badly mixed. I have gone over the *Carex* sheets and the incorrectly named ones are there in abundance. I am sure that a similar condition exists in the other genera.

In studying these old species and genera, I try to put myself in the place of the author, and where he had what has turned out to be a mixture, I try to find out what he had primarily in mind, and to that element I apply his name. Where he has given a description, I apply his name to a plant answering his description, except where he gives clear indications that this is not the proper course. Where he has given no description, but based his species on references to older authors, some having plates and others not, I know from my own experience in similar matters, that in all probability he was many more times influenced by the plates than by the older descriptions and therefore in such cases names are ordinarily applied in accordance with the plates. Specimens in his herbaria are often of the greatest possible value in doubtful cases, but what an author wrote must always be

given preference. The method pursued is the type-method. It is the method which has been in use for many, many years by many botanists in one way or another. Calling it the type-method merely emphasizes the need of some definite procedure in this class of investigation; and the rules of the type-method simply point out the best criteria for use in trying to find out what authors have really had in view. These rules when properly understood and applied are in no sense arbitrary, but are a wonderful help in arriving at results which are not arbitrary, but which best express what previous authors had in mind. The statement that older authors did not work with types is entirely wrong. They could not have done any work at all if they had not had material before them with which to work; and from such material, whether a plant in the field, a specimen in the herbarium, a plate or a description from a previous author, do we select what best expresses an author's view and call it a type.

Concerning *Solidago rigida*, I am sorry that Mr. Weatherby fails to let us know what he thinks the plate of Hermann represents. He surely does not identify it with the "*Solidago rigida*" of the manuals. It is in fact an unusually excellent illustration, as far as foliage is concerned, of the plant with which I identified it. That plant does have stem-leaves to which the phrase that the leaves are "as if embracing the stem at the base" does apply, as Mr. Weatherby could easily find out on investigation. In fact this phrase applies better to that plant than it does to the *Solidago rigida* of the manuals. That plant as is well known also does have forms in which the racemes are not recurved but erect and fascicled. Hermann in order to illustrate a tall plant without bending on a small page, cut off parts of his specimen both at the bottom and at the top, as he did with many others. His illustration shows undeveloped flowers or small clusters far down the branchlets from the developed flowers shown—a result probably arising from growing American plants under European conditions. (See Gray Proc. Am. Acad. 17: 163. 1882.)

Anyone comparing *Solidago rigida* of the manuals with this plate with any degree of care at all, would not attempt to justify their identity, and I do not understand that Mr. Weatherby makes any such identification. He is merely vague. The Linnaean name is taken from Hermann's name. All his citations refer to the same thing. His polynomial semi-descriptive name in the Hortus Clifortianus is evidently based on Hermann's plate and description as

every word is applicable, just as many of his names and descriptions are based on old plates and descriptions. He cites this name in the *Species Plantarum* with one word changed just as he and other older authors did constantly in citing names. The species is not listed among those which grew in the Clifftonian Gardens in the days there of Linnaeus (*Virid. Cliff.* 85–86), and it is not probable that anyone can now tell how the specimen labeled *Solidago rigida* in the Linnaean herbarium came there or whether it was in existence when he wrote the *Hortus Clifftonianus*. The proper thing to do is to apply definitely his name to the plate and descriptions he cited, and that requires, as I previously pointed out, the use of the name *Solidago rigida* for the plant which has been called *Solidago patula* Muhl.¹

It so happens that I have dealt with the purple-flowered *Eupatorium* question further in another paper, prepared before Mr. Weatherby's paper came to hand. His views on the International Rules seem either "wholly without authority" (whatever that means), or a good illustration of how hopeless those rules are to interpret. He is a firm believer in the arbitrary method. No rules of nomenclature are needed by adherents of these views. All that is required is a mere list of names with the ukase that these names and none others shall be used. Anything more is needless.

Let me say before concluding that after checking up a great many names, I have been more and more impressed with how well and how definitely the vast percentage of them have been applied and how well and how definitely the principle of absolute priority quickly works out as compared with that vague and indefinite thing known as usage.

I am sorry indeed to note that Mr. Weatherby has seen fit to sneer at a study of the older authors and to term such investigations "archaeological." I would recommend all botanists to study these old works, and I would especially recommend to members of the New England Botanical Club the need for the broadest kind of study. These old books are full of useful information of all kinds. Their

¹ In this connection, it is to be noted that in the Linnaean herbarium *Aster novae-angliae* L. is represented by *Aster grandiflorus* L.; *Aster cordifolius* L. is represented by *Aster divaricatus* L. (*A. corymbosus* Ait); *Aster Tradescanti* L. is represented by *Aster paniculatus* Lam.; and *Aster Novi-Belgii* L. is represented by *Aster puniceus* L. and by *Aster paniculatus* Lam. In all of these cases, Gray went back into the old synonymy cited by Linnaeus, found the starting point for each species, and applied the Linnaean names accordingly. In each of these cases, he disregarded the specimens in the Linnaean herbarium. In other words, in each one of these cases he did exactly the same thing which I did in the case of *Solidago rigida* L. (Gray, *Proc. Am. Acad.* 17: 164–8. 1882).

authors were often not able to describe floral structures well and often failed to illustrate them well, but they made up for this by much more fully and carefully studying other parts of the plant. The advent of the Linnaean sexual system of classification had a very bad effect on the study of all parts of plants except the flowers, and that has to a very considerable extent persisted to this day. It is very noticeable in our botanical manuals. But much of this other information is well brought out in these older authors. And I never go over their pages without a greater respect for their labors and learning and their desire for knowledge and their desire to impart it. I never feel like sneering at such work.

MAPLEWOOD, NEW JERSEY.

EQUISETUM PRATENSE IN BERKSHIRE COUNTY, MASS.—*Equisetum pratense* Ehrh. has apparently never been reported from Berkshire County, Mass. and its actual occurrence there may be worth recording.

It adds one more to the long list, well known to any field botanist, of interesting things found while stopping for lunch. During such a stop in the course of an automobile excursion, in the valley of a small tributary of the Blackberry River in the township of New Marlboro, my attention was attracted by some lustrous-leaved willows, probably *Salix serissima*, in a nearby thicket. Investigation showed that the thicket also contained a small but vigorous colony of *Equisetum pratense*. A specimen will be deposited in the herbarium of the New England Botanical Club.

E. pratense is known from three stations in the Housatonic valley in Connecticut, the northernmost within three miles of the Massachusetts line. There seems to be no reason why it should stop there; although a species of rather scattered and discontinuous distribution in New England, it may be hopefully looked for in the Housatonic valley in Massachusetts.—C. A. WEATHERBY, Gray Herbarium.

The dates of issue of the December and January issues (unpublished as this goes to press) will be announced later.